

and the nature of which may be inferred from the fact that I advised the author to withdraw his paper and submit the subject to a renewed investigation, with the aid of improved methods.

With regard to the question, which Mr. Lowne raises, as to my competency to form any opinion at all, on the ground that I had not myself devoted any special attention to the literature of the subject, I may remark that the points which had practically to be decided were (1) whether Mr. Lowne's statements were in themselves probable, and (2) whether they were corroborated by his preparations. Had I not felt myself qualified to form an opinion on these points I should not have accepted the reference.

E. A. SCHÄFER

The Late Prof. Clifford's Papers

In the "Mathematical Papers" (pp. 628-37) I was able to print the syllabuses of a series of ten lectures delivered by Prof. Clifford to a class of ladies at South Kensington in the spring and summer of 1869. Whilst turning over a collection of miscellaneous papers, in a box, Mrs. Clifford and I had the good fortune to light upon a manuscript quite ready for printing, and this ("Mathematical Papers," p. 628) subsequently formed part of the volume on "Seeing and Thinking;" but we could not find any trace of any more manuscript of the above-mentioned series of lectures. Just before the recent Easter holidays Prof. Karl Pearson returned to me a few pages of manuscript bearing on the International Scientific Series volume which I had lent him, and with them he sent me a large note-book which had been in the late Prof. Rowe's hands. On opening this book I at once saw that it contained very full notes of other lectures of the course. In fact, Lecture II. ("On Plane Surfaces and Straight Lines") is quite ready for press, as is also, I think, Lecture III. ("On the Rotation of Plane Figures"); Lecture IV. ("Of Similar Figures") is a fragment, and still more fragmentary is Lecture V. ("The First Principles of Calculation"). Of Lecture VI. ("The Theorem of Pythagoras") there are two loose sheets of figures: on one sheet is "the Bride's Chair," and the figures on this and the other sheet show that my information was correct, and that the remarks on pp. 633, 637 are *ad rem*. As Lecture IX. ("On the Shadows of a Circle") is very fully illustrated in the recent volume edited by Prof. Pearson, we see that we are in possession of a fairly complete presentment of Prof. Clifford's views on the subjects of the course of lectures.

Messrs. Macmillan have stated their willingness to publish the MS. of the second part of "The Elements of Dynamics," and I hope to be able, after a re-examination of it, to put the work into their hands for printing. When this book is got out, and the above lectures published in some shape yet to be determined, the mathematical world will be in possession of all that we can now look for from the hands of this great master.

University College School, April 25

R. TUCKER

Sir Wm. Thomson and Maxwell's Electro-magnetic Theory of Light

SHORTLY after writing my former letter I saw a copy of the verbatim report of Sir Wm. Thomson's lectures in Baltimore, and would have written to you to that effect and to apologise to Mr. Forbes for having doubted the accuracy of what I thought was his report, only that I met him in London about that time, and he then desired me not to do so. Sir William Thomson has now himself stated that the passage is correctly quoted, and I can only regret that he has expressed himself in the way he did.

I certainly think that anybody reading the passage would imagine that the velocity of propagation of electro-magnetic disturbances upon Maxwell's electro-magnetic theory of light, which he showed to be the same as the velocity of propagation of light, and to be a true velocity of wave-propagation—any one, I say, would suppose that this was the same thing as that Sir Wm. Thomson calculated in the year 1854.

Sir Wm. Thomson certainly says, "That is a very different case," but the rest of this sentence is rather ambiguous as to what the "it" after "putting" refers to, and I am afraid that many people will imagine that, in Sir William Thomson's opinion, Maxwell has made some unjustifiable assumption. I believe, however, that all he thinks is that Maxwell has not made a satisfactorily definite thing of the so-called electro-magnetic theory of light.

In Sir Wm. Thomson's article in Nichol's "Cyclopædia"

he puts the matter very clearly indeed. He says:—"The law of this phenomenon [transmission of electric signals] is identical with that which Fourier . . . found as the law of propagation of summer heat and winter cold to different parts of the earth," *i.e.* it obeys the laws of a diffusion and not of a wave-propagation; and again:—"Now it is obvious from these results [experimental results] that the supposed velocity of transmission of electric signals is not a definite constant like that of light:" and afterwards he says that, when an initial current is started, the potential rises simultaneously at all points, and that the apparent velocity would depend on the delicacy of our instruments. All these obviously distinguish between the propagation of a variable current in a conductor and a true wave-propagation.

He has also clearly pointed out a direction in which to look for a true wave-propagation. It will make his position clearer, and also Maxwell's, to use his analogy between water in an elastic tube and a conductor of electricity. I will suppose the water contained in a tube bored out of a very large lump of india-rubber. He enumerates three electric qualities concerned, and their hydrodynamic analogues:—(1) "Charge" or electrical accumulation in a conductor subjected in any way to the process of electrification. (2) "Electro-magnetic induction" or electromotive force excited in a conductor by variations of electric current. (3) Resistance to conduction through a solid. The hydrodynamic analogues are:—(1) Accumulation of a greater or less quantity of water in any part of the canal or tube. (2) Inertia of the water. (3) Viscosity or fluid friction. He explains that a true wave-propagation arises from the compressibility of the water, combined with its inertia, and that if the tube be elastic, like india-rubber, there would also arise a wave-propagation. "Accordingly," he says, "a definite velocity of propagation of electric impulses, depending on the inertia and the capacity for charge, is to be looked for, as has been done in a first article, published by Kirchhoff, on the subject."

Now, in all this discussion Sir Wm. Thomson omits to mention the only thing that is at all analogous to Maxwell's propagation of wave disturbances in non-conductors, and it arises from his considering the water as contained in a tube like ordinary india-rubber tubes, instead of in a tube bored in an indefinitely large lump of india-rubber. If we consider this case it is evident that one of the conditions to be considered is the propagation of waves in this lump of india-rubber. In Sir Wm. Thomson's tube there would of course be a velocity of wave propagation in the india-rubber, but that is a very different matter from the propagation of disturbances away from the neighbourhood of the tube by which energy would be carried away from it. To do this Sir Wm. Thomson should have included the propagation of sound in the air or whatever he supposed surrounding the outside of his tube. Without including this, he was not including anything a bit analogous to Maxwell's electromagnetic theory of light. In Sir Wm. Thomson's tube the whole state of affairs at any time could be expressed in terms of variables that represented bodies near the tube, while in the other case it would be absolutely necessary to introduce variables representing every part of the india-rubber which I have supposed of indefinite extent. This is just the difference between Sir Wm. Thomson's and Maxwell's views. According to Maxwell's view there is a great deal more going on outside the conductor than inside it, and it is evident that the inertia of the water is a very bad analogue to electromagnetic induction, for this latter depends essentially upon the form of the circuit, and not only upon its section and length. Maxwell has shown that light may be a wave-propagation of what are on his theory *analogous*, though probably utterly *unlike* the distortional waves propagated in the india-rubber, and has shown that a medium which would only transmit disturbances analogous to these would explain electric and magnetic phenomena. It is to be remembered that Maxwell's theory gets rid of all action at a distance, and that the only *experimentum crucis* between theories of action at a distance and of action through a medium is that in this latter case the energy may be propagated in time through the medium, while in the former it cannot.

I cannot conclude without protesting strongly against Sir Wm. Thomson's speaking of the ether as *like* a jelly. It is in some respects *analogous* to one, but we certainly know a great deal too little about it to say that it is *like* one. May be Maxwell's conceptions as to its structure are not very definite, but neither are any body's as to the actual structure of a jelly, and there is no real difficulty in supposing a medium whose condition is

represented by symbols that obey the laws that Maxwell has shown should be the laws of symbols representing the condition of a medium that would explain electric and magnetic phenomena. It seems very unlikely that any jelly is at all like the ether that Maxwell supposes. It seems much more likely that what he called "electric displacements" are changes in structure of the elements of the ether, and not actual displacements of the elements. He guards against this being supposed a necessary part of his theory when he defines polarisation in terms that certainly require a change of structure rather than a change of position, so that I think the word "displacement" was unfortunately chosen. I also think that Sir Wm. Thomson, notwithstanding his guarded statements on the subject, is lending his overwhelming authority to a view of the ether which is not justified by our present knowledge and which may lead to the same unfortunate results in delaying the progress of science as arose from Sir Isaac Newton's equally guarded advocacy of the corpuscular theory of optics. GEO. FRAS. FITZGERALD

40, Trinity College, April 25

The April Meteors

IN 1882 the Lyrid meteor shower (epoch, April 19-20) was noticed to be far more conspicuous than usual. The display had been quiescent for some years; it appeared to have degenerated into a third rate shower, scarcely deserving the trouble of observation. But in the year mentioned the stream gave distinct intimation of greater intensity, and showed the necessity of continuing annual observations of periodical showers such as this, even though they may exhibit, during a comparatively long interval, but a very feeble sustenance of the richness recorded in former times.

On April 20, 1882, Mr. Corder, at Chelmsford, watching for three hours between dusk and 12h. 30m., counted 26 Lyrids and 8 meteors belonging to other contemporary radiants of minor character. He regarded the horary rate of apparition on that occasion as about two or three times as high as on any other occasion since 1877, when he had been able to watch for the display. He found the radiant at $268^\circ + 37^\circ$, and remarked that four of the meteors seen were as brilliant as first-magnitude stars, but he had never found them a very interesting species in respect to their visible appearances.

No observations of the shower were obtained in 1883, the moon being near the full at the time of its occurrence; but in 1884 the conditions were more favourable. On the night of April 19, in the hour preceding midnight, 17 Lyrids were observed by the writer at Bristol. During the last quarter of an hour of the watch the sky was much clouded, and only one Lyrid seen. The horary number was computed as 22 for one observer—evidently, therefore, the display of 1884 was a notable one, and it was very unfortunate that a clouded sky prevented the development of the shower being watched through the morning hours of April 20, when possibly it may have attained a richness without parallel in late years. On the evening of April 20 it had evidently become exhausted, for in a watch of twenty minutes not a single shooting-star appeared, though the sky was very clear.

The present return of the Lyrids occurred under very auspicious circumstances. The moon offered no impediment to morning observations, which is far the best time to watch for these meteors, as the radiant, west of α Lyrae, is very low in the evening hours. The nights of April 17, 18, 19, and 20 were cloudless throughout, and on the three latter dates observations were made here with the following results:—

Date	Period		Duration of Watch	Meteors seen	Lyrids seen	Radiant Point
	h. m.	h. m.				
April 18 ...	12	0 to 14 30	2½	16	6	260 + 33½ ¹
" 19 ...	10	30 to 14 0	3½	26	10	267½ + 33 ²
" 20 ...	11	30 to 15 30	4	39	14	274 + 33½ ³
Ap. 18-20 ...	10	30 to 15 30	9½	81	30	267½ + 33½ ³

After April 20 moonlight and cloudy weather effectually prevented further work.

The table furnishes us with some interesting facts. It shows that during the three nights the proportion of Lyrids to uncomfortable meteors was very nearly the same, namely, as 3 to 5, and that the horary rate of their apparition was little more than 3. It also shows a very marked displacement of the radiant (in

¹ Sky clear; slight haze.

² Ib.

³ Sky very clear.

the direction of east longitude) from night to night. I regard this as the most interesting and certain feature observed. The three centres resulting from the paths of short meteors, observed with the utmost care, may each be relied on as very accurate. It therefore appears most conclusive that the radiant point of the Lyrids, similar to that of the August Perseids, increases in right ascension from night to night, and the extent of this displacement is even greater for the Lyrids than for the Perseids.

On April 19, 1884, 11½h., I found the radiant at $269^\circ + 33^\circ$, and on April 19, 1885, 12½h. (the middle time of the observation), at $267½^\circ + 33^\circ$. Allowing for the difference (about 12') in the sun's longitude at the two epochs in applying the correction for the displacement of the radiant (7° of R.A. daily) observed this year, we shall find that the two positions are in exact agreement.

The recent display has been decidedly meagre in point of numbers. There is a great falling off since last year, when the horary rate was nearly eight times as great. But some of the meteors observed this year were very bright and in a great measure compensated for their scanty apparitions. It is curious that three of the most brilliant Lyrids, equal to or exceeding Jupiter, appeared in nearly the same region of the western boundaries of Virgo. On April 18, at 12h. 57m., one of these fell, with a bright flash and streak, 10° east of Spica Virginis. Another on April 20, at 13h. 14m., came out very suddenly 7° above that star; and a third, at 14h. 1m., descended, with a swift, diving motion, about 13° east of the star, so that the path was nearly similar to that of the first of the three. These five meteors gave transient

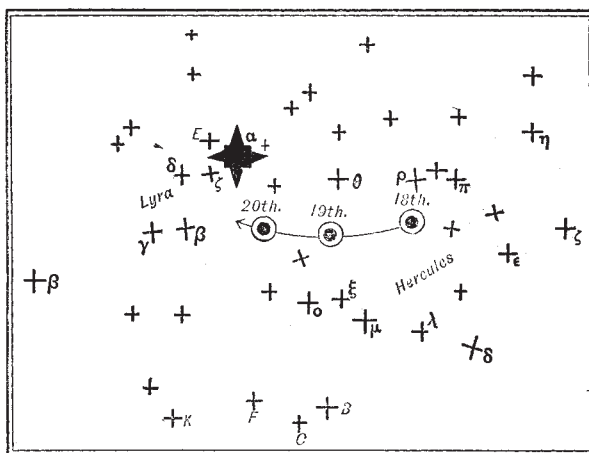


Diagram exhibiting the displacement in the radiant point amongst the stars of Hercules and Lyra on the nights of April 18, 19, and 20, 1885.

flashes of yellowish-white light, which brightly illumined the mist lying along the horizon, and left short streaks of very brief duration. I saw four other Lyrids, quite equal to first-magnitude stars, and these, together with the fainter members of the same shower, were nearly all registered amongst the stars surrounding Lyra, especially those of Hercules, Draco, and Aquila. Altogether I regard the April display of this year as one of considerable importance, for though much less rich than in 1884, it furnished meteors of greater interest and brilliancy. The low position of the radiant during observations last year may have influenced the visible aspect of the meteors from Lyra, especially as regards the swift, flashing characteristic so invariably noticed this year in the brighter members, or, possibly, the explanation may be that near the richer portion of the stream the corpuscles are smaller in almost the same ratio as the increased condensation. It will be advisable to regard this question during future observations of this shower, and especially the new feature detected this year as to the evident shifting of the radiant point from night to night. I believe the maximum of the shower is usually attained with the radiant at $269½^\circ + 33^\circ$ (the radiant point of its allied Comet I. 1861 is $270½^\circ + 32^\circ$). If the display extends over as long as seven days, and the radiant shifts 7° in R.A. every day, it is curious that the meteors will be *Herculids* on April 17, 18, and 19, *Lyrids* on April 20, 21, 22, and 23, and then *Cygnids* on April 24.

Contemporary with the special periodical display of this epoch there are vast numbers of feeble systems which annually give